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ECONOMIC THEORIZING AND SCIENTIFIC PROGRESS

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Since the organization of the American Economic Association thirty years ago, there have been but two of its twenty-five scientific meetings at which one session, at least, has not been devoted to theoretical discussion.

This prominence reflects the continuing interest of American economists in social speculation. The conspicuous place of theoretical papers in journal literature, the considerable proportion of expository treatises among formal publications, are counterparts of the same phenomenon.

The degree of absorption has varied from time to time. In 1880 Cliffe Leslie, echoing Dunbar, summed up the history of American political economy in the sentence "the United States had done nothing towards developing the theory of political economy."¹ The reflex of the animated controversies as to scope and method among English economists of the late seventies and, more notable, the arrival and activity in the United States of a group of German-trained scholars effected a virtual renaissance in American economic thought in the early eighties. The dominant characteristics of the phase were an avowal of the historical-inductive method and an election of concrete problems for study.

The historical movement reached its high-water mark in the United States in the formation of the American Economic Association in 1885. But in this achievement the tendency spent itself. Stirred by militant challenge, heartened by clearness of issue, supplied with convenient channels of publication, the group of thinkers to whom the work of Roscher, Cliffe Leslie, and Ingram seemed amendatory rather than revolutionary, drew to the fore. The impulse took form in the founding of the *Quarterly Journal of Economics* in 1886, with Dunbar's fine inaugural on "The Reaction in Political Economy" sounding the keynote.

The doctrinal controversies of the next half dozen years centered about the contributions of Marshall and Walker. Not since Ricardo's time had such coherent theories of value and distribution been enunciated. The gap which the discrediting of the wage-

¹ "Political Economy in the United States," in *Fortnightly Review*, October, 1880; reprinted in *Essays on Political Economy* (2d ed., Dublin, 1888), p. 128.

fund theory had left was filled by attractive formulae, and recognition of the entrepreneur function and its reward met the demands of modern industrialism.

Marshall's *Economics of Industry* had appeared in 1879 and Walker's *Political Economy* in 1883, without appreciable effect. Now, however, Marshall's election to the chair of political economy vacated by the death of Fawcett called attention to the novel elements in his doctrines; while Walker, aroused by Sidgwick's strictures, reiterated in incisive form the controverted elements of the *Wages Question*. The publication of Marshall's *Principles of Economics* in 1890 marked the climax of the debate. With it and the discussion immediately tributary, a lull was reached in economic controversy.

But the respite was brief. The same year, 1890, in which Marshall's *Principles* was issued, the English translation of Böhm-Bawerk's *Capital and Interest* saw light,—heralded indeed a year before by Bonar's admirable critique of the Austrian school. In quick succession followed Böhm-Bawerk's *Positive Theory of Capital* and Wieser's *Natural Value* in English translation, with Smart's handy *Führer* as a résumé. An outburst of controversial activity ensued, unparalleled in the history of American economic writing. During the nineties this sustained contest—exhibiting every variety of intellectual effort from stimulating analysis to hypercritical dialectic—raged about the Austrian doctrines.

Through sheer exhaustion of disputants—and witnesses—the struggle had abated when in 1899 Clark's *Distribution of Wealth* summarized with rare amenity of form the speculations of a profound philosopher and an inspiring teacher. The effects were wide conversion and ardent discipleship. For a decade Clark's theories dominated economic philosophy in the United States, weakening only with increasing resort to realistic studies and growing uneasiness as to the value of theoretical inquiries.

Walker, Böhm-Bawerk, and Clark thus constitute the major episodes in the thirty years of American economic speculation. There have been minor incidents,—the debate over the scope and method of sociology in 1895-98, the discussion as to the order and form of economic instruction in 1912—but these stand in immediate relation to local or temporary events.

The scientific results attained in the quarter century have not been commensurate with an incessant doctrinal activity. The dev-

otees of a science are notoriously incompetent to pass judgment upon its contemporary phase. There is likely to be either unwarranted depreciation or complacent overestimate. This is as true of the physical as of the social sciences. If, in our own day, Jevons reduced "the mazy and preposterous assumptions" of the Ricardian economics to "the fragments of a shattered science," so, in medical science, Osler has spoken of "an art which is still deep in the sloughs of Empiricism."²

But the depression which many American economists feel as to the present state and tendency of the science is something more than this habitual depreciation. It is born of the conviction that economic science as pursued in the United States has been infected by unsound method, that the productivity of scientific labor has been thereby impaired, and that a further diversion of energy into barren and profitless areas is threatened.

Following the example of the great masters of our science, influenced by the teaching of formal methodologists, American economists who have devoted themselves to economic speculation in the past thirty years have used a dominantly deductive method.

There can be no quarrel with this procedure. It has the advantage of precedent on the part of economists from Dugald Stewart to Alfred Marshall; of approval on the part of logicians from Archbishop Whewell to Professor Keynes; of successful resistance to methodological attack from Richard Jones to Gustav Schmoller.

The real charge against American economic speculation is not that it has made use of deduction but that it has made use of it wrongly and defectively. The indictment lies, in strict accuracy, not against erroneous method but against imperfect employment. The justice of this criticism can be best tested by projecting the evidence to be reviewed against the background of accepted, even though familiar, opinions as to scientific method.

The material of any positive science is a body of related phenomena subject to uniformity as to identity and sequence. Scientific progress consists in the increasing formulation of such uniformities as hypotheses, in tentative statement of them as theories, and in ultimate demonstration of them as laws.

The orderly procedure whereby this end is sought, we have been

² "The Army Surgeon" in *Aequanimitas* (2d ed., Philadelphia, 1906), p. 112.

taught for years, is the method of science,—not of *a* science but of *science*. In complete form it consists of three steps: observation, inference, verification. Observation and inference give us hypotheses; verification converts the tentative formulations into theories and ultimately into laws. To those whom the gods elect, the hypothesis comes lightly by intuition or imagination; to coarser clay the way is steep and hard,—by induction from assembled particulars, by deduction from admitted or established generalizations.

Marked differences of opinion—accented sometimes into acrimonious controversy—have developed at intervals as to the relative order and prominence of the several parts of the inferential process. Deductive, inductive, historical, comparative, statistical, mathematical, are terms descriptive of the varying emphasis. The outcome has been virtual agreement that there is a constant alternation of the processes of induction and deduction in the actual conduct of scientific inquiry, and that in this “interlacing” historical experience, comparative elimination, statistical uniformity, and mathematical ratio have part.

Whatever difference of opinion may have prevailed as to the order and emphasis of the several elements in the reasoning stage of scientific speculation, there has always been agreement that the third step, verification, formal and deliberate—“the process of testing, by means of an appeal to facts, the validity of the conclusions already arrived at by a course of deductive reasoning”³—is an indispensable requisite in the transition of an hypothesis into a theory and of a theory into a law. Without it speculation is an intellectual gymnastic not a scientific process. Mill likened the distinction to that which exists between the Aristotelian physics and the Newtonian theory of the heavens.⁴ In our own speech it is the contrast between the political arithmetic of Petty and the statistical studies of Pearson. The chemist, the physicist, the physiologist, venturing to propound a new theory without confirmatory evidence, is discredited as an investigator and laughed out of court as a scientist. If recourse cannot be had to observed facts for verification—as often in certain of the physical sciences, astron-

³ Fowler, *The Elements of Inductive Logic* (3d ed., Oxford, 1876), p. 244. So too Dunbar (*Quarterly Journal of Economics*, I, 5): “The verification of results reached by deductive reasoning should call for as patient collecting and as conscientious sifting of facts as any other use of observation.”

⁴*System of Logic* (2d ed., London, 1846), I, 579.

omy and geology—the hypothesis remains hypothesis, plausible, perhaps, but tentative none the less until the disclosure of additional facts by some new instrument of precision, by some abrupt discovery of evidence, affords material for the lacking test. The investigator “makes intellectual keys,” declares a recent writer upon scientific method, “and then tries whether they fit the lock. If the hypothesis does not fit, it is rejected and another is made.”⁵ That the scientific workshop is full of such “discarded keys,” Pearson reminds us, was stated finely even a generation ago by Faraday: “The world little knows how many of the thoughts and theories which have passed through the mind of a scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examination; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions have been realized.”⁶

The question remains whether formal verification—however essential in the physical sciences—be either a possible or a necessary element in economic inquiry. As to the needlessness of formal verification, the implication seems to be that the economic investigator is equipped with such a range of general information that automatically he will reject a hypothesis not in harmony with the facts. This smacks much of the disposition of the uninformed to declare that the frequency distribution of two groups of data is identical because the curves look alike.

The consideration as to the impossibility of formal verification traces back to the contention of Mill⁷ that direct verification in the social sciences is impracticable because of the absence of “previous empirical generalizations” and because of the inability to compare the hypothesis with individual experiments or instances of like circumstance. But Mill was concerned less with the establishment of social uniformities than with the predictions of social theory, and even here he insisted that while direct verification is impossible, there is “an indirect verification, which is scarcely of less value, and which is always practicable,”—the process whereby the conclusion drawn as to the individual case is “verified indirectly, by the verification of other conclusions, drawn in other individual cases from the same laws.” Formal logicians have gone

⁵ J. A. Thomson, *Introduction to Science* (New York [1911]), p. 69.

⁶ Quoted in *The Grammar of Science* (3d ed., London, 1911), p. 32, and by Thomson, *op. cit.*, p. 26.

⁷ *System of Logic*, II, 588-90.

even farther in this process of qualification, without waiving the essential consideration: "Verification is not a distinct mode of proof," Fowler maintains, "but is simply a confirmation of one proof by another, sometimes of a deduction by an induction, sometimes of an induction by a deduction, and finally, sometimes of an induction or deduction by another."⁸ Proof—not any particular mode of proof—is the essential requisite of scientific thought.

All said and done, the consensus of opinion among logicians and scientists may fairly be described as follows: The economy of intellectual effort demands that any hypothesis as to an existing uniformity or a causal relation among a body of facts shall before presentation as a scientific contribution be verified by experiment, by specific instance, or by logical equivalent. If experience be the necessary resort, the mere circumstance that the facts are complex or difficult of access does not relieve the investigator of his responsibility. He must exhaust the possibilities, state frankly what he has done, and qualify his inference accordingly.

We are now prepared to formulate the indictment: Economic speculation in the United States has been marred by the omission, deliberate or unwitting, of verification,—the comparison between the conclusions of reasoning and the results of observation. In so far, it has partaken of the character not of economic theory but of economic theorizing.

In passing to a bill of particulars, the critic suffers the woe of an academic ingrate mingled with the remorse of a doctrinal patricide. The challenged are the *dei majores* among American economists of our generation,—the men whom we delight to honor as scholars and as teachers. If the sword be now raised against them, it is in no spirit of filial impiety but in the sure faith that they who have taught us to forge its strength and to whet its edge will be the first to cry that it be wielded if true cause arise.

From out the mass of theories enunciated by American economists of the present generation I have selected four notable doctrines,—notable in the sense of formal issue, wide diffusion, critical discussion, and doctrinal influence:

Henry C. Adams's Theory of Constant Returns,
Simon N. Patten's Theory of Consumption,
John B. Clark's Theory of Wages,
E. R. A. Seligman's Theory of Tax Incidence.

⁸ *Elements of Inductive Logic*, p. 249.

Henry C. Adams's theory of constant returns was given full expression in 1887 in a compact monograph, "The Relation of the State to Industrial Action,"⁹ as part answer to the query (p. 54) "Is there any principle according to which industries may be classified so that the statesman can easily determine what lines of business should be brought under the rule of public financing?" Rejecting the *laissez-faire* device of delay until actual experience of ill, Adams proposed by "the scientific analysis of industrial relations to determine with some degree of accuracy" the nature of this principle. The subsequent argument can be set forth by direct citation (p. 55): "All industries, as it appears to me, fall into three classes, according to the relation that exists between the increment of product which results from a given increment of capital or labor. These may be termed industries of constant returns, industries of diminishing returns, and industries of increasing returns. The first two classes of industries are adequately controlled by competitive action; the third class, on the other hand, requires the superior control of state power."

Then passing specifically to the composition of the first class (p. 55): "All those businesses in which success depends largely on attention to detail, and where the personal element of the laborer is brought prominently into view, fall under this class. For example, the retail business of merchants is subject to the rule here stated. . . . There can, therefore, be no motive for endeavoring to apply the rule of public financing to businesses of this sort."

Professor Adams has here conformed to the first two stages of scientific reasoning, observation and inference; he has ignored the third, verification. From the days of Ricardo and Senior, industries of diminishing and of increasing returns have been recognized as valid corollaries of the law of diminishing returns and the principle of monopoly gains. The recognition of an intermediate group subject to a law of constant returns and the assignment thereto of the great body of industrial enterprises constituted a distinct hypothesis. However legitimate as a tentative assumption, attained by elimination or deduction, such an hypothesis could be properly set forth as an economic law only after the amplest proof by reference to actual facts,—in this particular case both existent and accessible.

Simon N. Patten's theory of consumption, enunciated in *The*

⁹ *Publications of the American Economic Association, First Series, Vol. I, No. 6.*

*Consumption of Wealth*¹⁰ in 1889, is a bold and courageous venture exhibiting in his most characteristic speculation the form and mode of the author's reasoning process. The starting point is the proposition that (Introduction, v) "while production is determined by consumption; the latter is itself largely modified by the former." The problem is explicitly conceived as (*ib.*) "to bring all those facts into harmonious relations to one another which throw light upon the laws regulating the consumption of wealth." Deduction is elected as the mode of investigation since the data are primary facts of human nature and social organization, and warning is served that the ultimate laws are likely to be obscured by disturbing causes and counter forces.

But the method pursued and the result attained fall short of such formal avowal of plan and purpose. By indulgence in conjectural history, "tracing the past development of man" (p. 9) or going "back to primitive times" (p. 24), by resort to free speculation unchecked by attempted proof, Professor Patten gives us brilliant hypotheses and stimulating conjectures,—but not established principles.

In declaring, as a primary or ultimate law of consumption that (p. 13) "With every increase in the variety of our food, the intensity of pleasure derived from the food must be reduced; so that, when the system becomes accustomed to the new diet, the point of satiety will be reached sooner than was formerly the case," or in stating as the economic, in contrast to the natural, order of consumption that (p. 19) "A man under economic conditions will first choose that article which has the highest ratio of pleasure to pain, and then other articles will be chosen in that order which allows the highest ratios to stand first," there is no trace of that deliberate reference to positive phenomena for verification, amendment, or rejection which constitutes an essential of scientific thought. If the propositions be true, they attest the profound insight, the rare intuition, the fertile imagination, and the signal good-luck of a gifted philosopher. Like Galileo he will have "used method by genius." If the propositions be false they afford new evidence that philosophical accident may not be relied upon to replace logical method, and that ordinary men can hope to rival Galileo only by using the rules which he could neglect.¹¹ To

¹⁰ *Publications of the University of Pennsylvania: Political Economy and Public Law Series*, No. 4 (Philadelphia, 1889).

¹¹ Cf. Case, "Scientific Method as a Mental Operation" in *Lectures on the Method of Science* (ed. T. B. Strong, Oxford, 1906), p. 11.

economic science the real injury is the complete uncertainty in which we are left as to whether the propositions are true or false.

John B. Clark's theory that the specific productivity of labor fixes wages embodies the essential concept of the author's economic system,—the doctrine of imputation. With rare intellectual tenacity Professor Clark has here constructed a "homogeneous hypothesis" as to the phenomena of economic distribution, not by reference to such facts, but by large reliance upon the legal concept and social institution of private property. As from the earliest recognition of private property in economic relations, man obtains the full fruit of his exertion as reward, so in the most highly developed industrial society the labor-unit tends to receive as wages an amount equivalent to the value of the product created by "a final unit of social labor."¹²

From the days of Aristotle analogical reasoning has been a recognized mode of inference. The geologist infers the efficient causes of events that happened in the Devonian age from the consequences of particular happenings today. Darwin formulated theories as to variations which might have occurred in unthinkable distant aeons from the particular variations which he observed in his domesticated pigeons and cultivated plants.¹³ But analogy, even though often the only recourse in reasoning from the present to the remote, from the accessible to the inaccessible, can never yield anything more than an hypothesis. Even then the way is beset with peril. "It is a common error," warned Rowland the great investigator, "which young physicists are apt to fall into to obtain a law, a curve, or a mathematical expression for given experimental limits and then to apply it to points outside those limits. This is sometimes called extrapolation. Such a process, unless carefully guarded, ceases to be a reasoning process, and becomes one of pure imagination specially liable to error when the distance is too great."¹⁴

With respect to contemporary social phenomena, the validity of an analogical hypothesis is established not by flawless inference and lucid exposition but by the bare hard test: Is it the simplest theory available for explaining the facts? Like the mechanical hypothesis of modern physical science it fulfills its purpose only

¹² *The Distribution of Wealth* (New York, 1899), p. 168.

¹³ Thomson, *op. cit.*, pp. 58-59.

¹⁴ "The Highest Aim of the Physicist" in *Johns Hopkins University Circular*, March, 1900, p. 19.

when its "conjectural attributes explain in the simplest and most plausible way the largest body of known phenomena and when they anticipate phenomena afterwards observed."¹⁵ The theory of specific productivity was propounded void of any such test. How signal was the omission is suggested by the brilliant attempt of an accomplished disciple eleven years later to repair the defect.

Professor E. R. A. Seligman's theory of the incidence of taxation, as presented in 1892 and revised in the successive editions of *The Shifting and Incidence of Taxation* in 1899 and 1910, may reasonably be described as the most ambitious attempt at economic deduction made by an American scholar of our generation. Prefaced by an imposing historical review of the literature of the subject—past and present, deliberate and incidental, domestic and imported—the essay should be free from at least the fundamental errors of method which the author's profound erudition and critical acumen have detected so unerringly in the writings of others.

If Ricardo's doctrine of incidence, "called *par excellence* the absolute theory," be incomplete in failing (p. 151) "to notice the practical effects of economic friction," with the result that (*ib.*) "It might, perhaps, be true so far as it went, but it would even then not go far enough to explain actual phenomena," the theory which is to replace it should certainly escape the same pitfall of defective method. Yet in this respect—the failure to submit conclusions to verification and test—Professor Seligman's analyses are glaringly Ricardian. His assumptions, it is true, are not the few simple premises of Ricardo, but a body of empirical generalizations that incorporate certain of the characteristics of the modern industrial system. But his conclusions are none the less unverified hypotheses.

Here and there detached facts are cited in illustration. Where "a careful statistical investigation" (p. 334), "a more recent inductive verification" (*ib.*), "a more recent investigation" (p. 335), are conveniently at hand—as in the case of the taxation of mortgages—the results are used to test the conclusion. But beyond this, and occasional bibliographical references to (p. 372) "inductive statistical studies" of the incidence of other taxes, there is no attempt to establish the validity of hypotheses by appeal to facts. The author, fully alive to the purpose of such procedure, seems to feel himself absolved by the adequacy of his premises, the certainty of his inferences, the guarded application of his conclusions, from

¹⁵ L. T. More, *The Limitations of Science* (New York, 1915), pp. 15, 27.

the necessity of undergoing the drudgery and detail that verification involves.

Grave as has been the direct harm to the progress of economic science by faulty method, the larger injury has been the unwholesome influence exerted by example upon students, devotees, and disciples. This has taken the form of (1) the encouragement of commentation, (2) the vogue of text-book writing, (3) the tolerance of metaphysical controversy as to final terms and ultimate concepts.

1. Historians of scientific thought describe as "the commentatorial spirit" that disposition among scholars to lean on the authority of earlier thinkers, "to study the opinions of others as the only mode of forming their own; to read nature through books; to attend to what had been already thought and said, rather than to what really is and happens." This "reign of mere disputation"¹⁶ has been recurrent in the history of thought, and always with unfortunate result. "The wavering abstractions, indistinct generalizations, and loose classifications of common language," records Whewell, were "the only source from which the schoolmen of the Middle Ages drew their views, or rather their arguments: and though these notional and verbal relations were invested with a most complex and pedantic technicality, they did not, on that account, become at all more precise as notions or most likely to lead to a single real truth. Instead of acquiring distinct ideas, they multiplied abstract terms; instead of real generalizations, they had recourse to verbal distinctions. The whole course of their employments tended to make them, not only ignorant of physical truth, but incapable of conceiving its nature."¹⁷

A kindred mode of intellectual activity has engaged American economists of our own generation to a very considerable extent. It has given us that "scientific snobbishness" which M. Poincaré warns us, "is so easy to gull by its novelties."¹⁸ It has exalted smartness and hypercriticism at the expense of scientific industry. It has constituted a mischievous concept, "the economic theorist" or the specialist in economic theory,—a superior breed of economist who looks upon positive inquiry much as the late Professor Clifford regarded sociology, as "all about papa and mama, clothes, houses, shops, policemen, half pence and such like."¹⁹ It

¹⁶ Whewell, *History of the Inductive Sciences* (London, 1837), I, 264.

¹⁷ *Ibid.*, p. 322.

¹⁸ *Dernières Pensées* (Paris, 1913), p. 241.

¹⁹ *Lectures and Essays* (London, 1886), p. 435.

has countenanced a mode of scientific participation wherein the economic fledgling, instead of serving the hard apprenticeship of observation, investigation, description, and empirical generalization, attempts the short-cut to scientific eminence by laying hold of the latest contribution to economic speculation, mulling and pawing over it by what Pearson has termed "some obscure process of internal cogitation," and emerging with "a paper on theory," with rejoinder and counter reply to follow.

2. Since 1885 there have been published in the United States nearly thirty textbooks on political economy. The text-writing habit of American economists is in part capable of materialistic interpretation,—the problem of undergraduate teaching and the pressure of enterprising publishers. In so far as an economic treatise is an exposition—in manner suited to student or reader needs—of the prevailing state of the science, with such further addition of amendment as the independent labors of the author have contributed, it serves a valid scientific end.

This ceases to be the case when the textbook becomes either a mere stylistic paraphrase or a device for incorporating untested hypotheses into the body of economic theory. The state of mind wherein the economist as teacher finds it essential, as evidence of intellectual autonomy, to prepare a textbook of his own composition, much as the economic departments of the last decade deemed it important to establish institutional journals, is of course unfortunate. But at worst this is prodigal waste of a scholar's energy. Far more serious in its scientific consequence is it for the textbook to be exploited in the exposition of scientifically incomplete theories. With the manual become in so far treatise, demanding that every new element undergo the hard rigorous test of scientific method, that gaps be neither evaded nor glossed but remain gaps until properly bridged,—substitution of hypotheses for theories, of assumption for proof, is a mischievous, because a masked, lapse.

3. It is no new thing for economists to engage in controversy as to final terms and in speculation as to ultimate concepts. The historic Political Economy Club discussed "the proper signification of the word Demand." John Stuart Mill rated as "an unsettled question of political economy" the correct meaning to be attached to "the words productive and unproductive." Malthus wrote a book on "Definitions in Political Economy," and appended a set of them. The last months of Ricardo's life were absorbed in

animated controversy as to the measure of value, and there is at least Maria Edgworth's story of the gentleman who, when asked if he would be of the Political Economy Club, answered that he would "whenever he could find two members of it that agree on any one point."

There are two motives which may lead the economist to such activity. The first is the desire of the investigator to amend or revise an accepted term or concept by incorporating the tested result of his own research. It is the manner in which the progress of a science is registered. Its characteristics are caution, restraint, and abundant evidence. Of this kind were Ricardo's correction of Gresham's law and Walker's disassociation of the captain of industry.

Very different is the impulse to debate terms and to discuss postulates in a spirit of logical disputation and metaphysical refinement, as exemplified in so much American economic writing. If there be reluctance in accepting as "an axiom in modern thought" that "the hope of discovering the laws of nature and our relation to them by metaphysical reasoning is impossible,"²⁰ there is surely agreement that this is not the initial task of the investigator. The physicist debates the constitution of energy, the chemist discusses the composition of matter, the biologist argues the origin of life; but none of these as the economist theorizes over, let us say, the nature of value. Historical equipment, intellectual maturity, inductive experience must be possessed by the natural scientist in the very highest degree before he attempts to grapple with the ultimate concepts of his field of study. "What is matter; what is gravitation"; asked Rowland, "what is ether and the radiation through it; what is electricity and magnetism; how are these connected together and what is their relation to heat? These are the greater problems of the universe. But many infinitely smaller problems we must attack and solve before we can even guess at the solution of the greater ones."²¹ Even then, the physicist does so in face of a growing protest. An acute critic of modern scientific tendencies, after lamenting that "many men of science have forgotten the distinctions between the creations of nature and the creations of their imagination," and insisting that "the question may well be asked, whether physical science has not outgrown a method proper for the alchemist and the astrologer," has attempted to draw "a definite

²⁰ More, *op. cit.*, p. 1.

²¹ *Op. cit.*, p. 17.

boundary line between the regions of physics and metaphysics" by saying that "the domain of physics is concerned with the discovery of phenomena and the formulation of natural laws based on postulates which are determined by experience and generally accepted as true; the causes of phenomena and the discussion of the postulates of science lie in the province of the metaphysician."²²

The distinction is too instructive to be lost. The term "metapolitics" to describe "an investigation of the speculative basis of political doctrines" is as old as De Lolme and Coleridge. So too, a "meta-chemistry," dealing with "the chemistry of the supersensible," and a "meta-physiology," concerned with the "principles outside of and higher than those of physiology and the material organism."²³ The Oxford Dictionary indeed countenances "the practice of prefixing 'meta-' to the name of a science, to form a designation for a higher science (actual or hypothetical) of the same nature but dealing with ulterior and more fundamental problems." Let there be, in like manner, a "meta-economics" concerned with the economics of the supersensible. Since we must theorize, let us, with Sir Lucius, preserve "the genteelest attitude into the bargain."

This constitutes no denial of the ultimate possibility, and the eminent desirability, of a theoretical economics. Hans Driesch prefaced his Gifford Lectures on *The Science and Philosophy of the Organism*²⁴ with the frank admission that (p. 3), "One may be a first-rate organic chemist even when looking upon the atoms as small billiard balls, and one may make brilliant discoveries about the behavior of animals even when regarding them in the most anthropomorphic manner—granted that one is a good observer; but it can hardly be admitted that our chemist would do much to advance the theory of matter, or our biologist to solve the problem of the relations between body and mind."

The economist may proceed farther and agree in paraphrase that (p. 4), "only by becoming a true philosophy of nature are social sciences of all sorts able to contribute to the highest questions which man's spirit of inquiry can suggest." But having gone so far the economist must continue at equal pace with the biologist and insist that, a "naïve" science becomes a philosophical speculation by transition, not by replacement; that (p. 5) "we perhaps

²² More, *op. cit.*, pp. 17, 30, 113-14.

²³ Oxford Dictionary, *sub nom.*

²⁴ Aberdeen University Studies, No. 33, Vol. I (Aberdeen, 1908).

shall be able to understand what its part may be in solving what can be solved," and that (p. 4) "The beginning of a long series of studies is not the right place to decide this important question."

If there be substantial truth in the foregoing, the moral to be drawn is neither methodological formulae nor an "advice to young economists," but a larger scientific consciousness. The best remedy for a half-science, Poincaré insists, is more science.²⁵ The political economist can not with impunity continue to neglect an essential process in scientific inquiry. So long as he does he must be prepared to face the biting query that the critic of the speculative tendency in physical science, to whom I have already referred, has propounded: "Is it any wonder that a gulf is growing not only between men of science and the rest of the world, but also between theorists and those who are still willing to submit their imagination to experience? Such a gulf is certain to continue so long as theorists are willing, and even anxious to ignore common sense and the facts gained by patient and exact experimentation."²⁶

Some realization of this danger has come. "My last book on economics was a failure," Professor Patten cheerfully recorded in 1912, "and having realized this I resolved to write no more books on economic theory until I could see some attainable goal."²⁷ On the other hand, the labors of a growing body of American economists—Taussig upon international trade, Ripley upon railway transportation, Kemmerer and Fisher upon prices, Moore and Barnett upon wages, Mitchell and Persons upon crises, T. S. Adams and Plehn upon taxation—to single out but a few—point to acceptance, conscious or unconscious, of Pearson's dictum that although the discovery of law is the peculiar function of the creative imagination, yet this imagination has to be a disciplined one. "It has in the first place to appreciate the whole range of facts, which require to be resumed in a single statement; and then when the law is reached—often by what seems solely the inspired imagination of genius—it must be tested and criticised by its discoverer in every conceivable way, till he is certain that the imagination has not played him false, and that his law is in real agreement with the whole group of phenomena which it resumes."²⁸

²⁵ *Dernières Pensées*, p. 241.

²⁶ More, *op. cit.*, p. 112.

²⁷ *The Reconstruction of Economic Theory*, p. 1.

²⁸ *The Grammar of Science*, p. 31.

The output till now is not large; but it is only in this wise that knowledge grows. In science as in affairs, "the recognition of our ignorance is the surest way to get rid of it."²⁹ Professor Clifford's modest dream of "the good time coming" in natural science was "when also benevolent old ladies may be expected to know one end of a guinea-pig from the other."³⁰ A generation ago Cliffe Leslie ventured, more hopefully, that America would take an active part in bringing to light and in solving those "new economic problems" which the chief economists of both worlds have never yet raised, and of which they have not dreamt.³¹ If the coming years are to bring us nearer to that still remote goal, it must be by unremitting regard to Huxley's classic warning: "Whenever science has halted or strayed from the right path, it has been, either because its votaries have been content with mere unverified or unverifiable speculation; or it has been, because the accumulation of details of observation has for a time excluded speculation."³²

²⁹ Clifford, *Lectures and Essays*, p. 96.

³⁰ *Ibid.*, p. 423.

³¹ *Essays in Political Economy*, p. 154.

³² "The Progress of Science, 1837-1887," in *Methods and Results: Essays* (New York, 1898), pp. 64-65.